

to the origin of the *strandlinjer* near the sea, I shall not follow out here. I shall only remark, with respect to the parallel roads of Lochaber, that Prestwich's hypothesis of landslips is untenable as soon as the alternation of rock-shelves and detritus-shelves is recognised. A different origin for the Scottish roads and for our *sefer* can hardly be supposed. ANDR. M. HANSEN

University Library, Christiania, December 17, 1885

The New Star near χ Orionis

THIS star, since December 20, 1885, has been very slowly decreasing in brightness. No trace of nebulosity was observed around it. Its colour on December 19 was red-orange, now it is yellow-orange. Its spectrum is of the 3rd order of Secchi. On December 19 it was very brilliant from the red to the blue, with six to eight brilliant bands decreasing in light to the violet, or the more refrangible side. Now the red of the spectrum is very dark, the yellow less luminous, and the blue more faint than before. The maximum of light is always in the green.

The apparent position of the new star, which I determined on December 20, 1885, 10h. om. 17s., Palermo M.T., was

$$\alpha = 5^{\text{h}}. 49^{\text{m}}. 4^{\text{s}}.54^{\text{s}}, \delta = 20^{\circ} 9' 4''.3.$$

Palermo Observatory, January 5

A. RICCÒ

Anchor Frosts

MR. J. HANDS, in NATURE of January 14 (p. 246), gives an interesting account of an anchor frost, "the most marked effects of which are," he says, "seen in comparatively still water." He adds, "it is said, that water coming upon it (anchor frost) from above will rise in level and flow over it, as over a solid obstruction. This I have not seen myself."

I have seen (and *felt*) this occur on at least one occasion, rather to my discomfort.

When in the Arctic, early one morning in late autumn, I went out to shoot deer for our winter stock of food, and forded a stream at a shallow rapid, dry-shod, in Eskimo boots coming up to the knee. The day, for the season, became very cold, and on my return homewards in the evening I found that the water in the rapid had risen so much that it came fully a foot above the tops of my boots, filling them with water. This increase of depth was wholly caused by the obstruction of a collection of ice-crystals in the form of wet snow, or pulp (through which my feet readily sank), adhering to the stony bottom of the stream. I had to hurry over the mile or two to my fireless tent, rip off my frozen boots and trousers, then jump into my blankets.

The position of this anchor-ice was the very opposite to that described by Mr. Hands, being in a swift but shallow rapid flowing out of a comparatively still deep pool frozen over with a thin coat of ice, but, where open, close above the rapid, having many small ice-crystals floating on and near the surface. The cause of these crystals coming into contact with, and adhering to, the stones at the bottom of the rapid I have given elsewhere, possibly in a previous number of NATURE.

The tenacity of cohesion of this soft mass of wet snow is more clearly indicated by its resisting the great force of a strong rapid, than where it occurs in comparatively still water.

JOHN RAE

4, Addison Gardens, Kensington, W., January 16

The Dover Boring

AS the deep boring at Dover is alluded to in the last number of NATURE (p. 255), it may be well to state that a short notice thereof was appended to my paper on the Chatham borings, which was read in abstract at the meeting of the British Association last year, and that a fuller account was included in my paper "On Deep Borings in Kent," read to the Geological Society a few weeks ago (December 2, 1885), and noticed in NATURE of Dec. 24, p. 190. Since then I have got some further information, and specimens of the deeper beds found are now under examination by Mr. J. Shorman, Palaeontologist to the Geological Survey. My paper being now in type, though not yet published, there is no need to enter into details, either of this or of a still deeper boring now going on in the same county. Any additional evidence as to the deep-seated rocks of Kent I hope to give in a supplementary paper, if needful, and I hope also shortly to visit the sites of the two borings referred to.

WILLIAM WHITAKER

33, East Park Terrace, Southampton

The Viper and its Young

I WAS very much interested in an account of a viper swallowing its young, given in NATURE a short time ago, and would like to corroborate the statement of Mr. Middleton's correspondent, anent this extraordinary performance, by relating as briefly as possible a little incident of which I was a witness. About the end of August 1885, I was watching a coolie underbushing in the bush on the Demerara River, Demerara, when suddenly a large labaria snake raised his head with open mouth just in front of the coolie. The man struck at it with his cutlass (a knife about twenty inches long), and afterwards assured me that he hit the snake, but I saw the reptile glide off towards the river. I pursued, but without success, the snake having doubtless taken advantage of the river as a means of escape. The coolie, who was a thorough bushman, having been born and brought up in the bush, told me that in all probability there would be another labaria not far off, as they always went in pairs. Next afternoon I heard that William, the coolie, had encountered another labaria, which he had killed. On my going to the place where he was at work, he told me he had buried the snake after cutting off the head. I may here state that a bushman almost invariably buries a snake after killing it, interring the head in a separate grave from the body. This the bushman does because, he says, if any one puts his foot on a snake's skeleton and a bone pierces the skin, the result will be nearly akin to the bite of the snake. This common belief among the sons of the forest has no doubt a good deal of truth in it, especially if a bone be broken in the wound. The interment of the head in a separate grave is merely done as a graphic assurance that the snake can never come to life again. Being curious to know if this labaria was the one I had seen on the previous day, I made William dig the reptile up, and was pleased to find it altogether a much thicker and shorter snake. On recounting his narrow escape from a bite, William vented his spleen by giving the snake's body a hack with his cutlass, when, to my astonishment, out through the wound came seven young ones, varying from five to ten inches in length, as nearly as I could guess. They were all quite lively, although covered with a sort of thin film or saliva. The largest of the brood seemed quite determined on business straight off, so I rapped him over the neck with the back of my knife. We killed the whole lot of them, and William carefully buried them, remarking that the bite of the young was quite as bad as the bite of the old snake. Now these snakes had been buried at a depth of eight or ten inches for from sixteen to eighteen hours, and on liberation did not seem a whit the worse for their entombment. William told me that when he saw the snake first it was lying coiled up fast asleep, and that he had nearly put his foot right on top of it; he, however, cut a stick and killed it before it awoke. We both agreed that from the appearance of the snake she had shed her skin only a few days before. This in all probability was the case, as I found a shed skin about three yards from where she had been killed. The little snakes seemed to me to be inside the stomach, and not in the gullet as Mr. Middleton suggests. The mother also was in a place where there was scarcely any likelihood of her ever having been disturbed to cause her to swallow her brood, and it will be evident that the young did not quickly perish even after the mother was dead. From what has just been stated, would it not be reasonable to advance the theory that the mother snake swallows the young ones after they are hatched, and retains them in her stomach until they attain a development that fits them to take care of themselves, when she either vomits them up, or they wriggle out of her mouth of their own free will?

Not being a naturalist, I am sorry that I cannot give any but the Indian name of this snake, but I will endeavour to find it out if you wish. The labaria must be well enough known to naturalists.

A CREOLE

White Blackbirds

A WHITE blackbird lived in our garden a year or so ago for about twelve months. Our gardener had seen it there, or at least one like it, before. I often saw it within a few yards' distance, and it was certainly three parts white, though presenting a mottled appearance. Some neighbours who heard us speak of it said it must be *their* white blackbird, meaning that they had seen it in their gardens. It was shy, but not more so than other blackbirds, and once, during sharp winter weather,

came close to the house and into the yard where the poultry feed. I have not seen it for some months, nor heard of it in any other garden.

A. S. MATHEWS
Edgbaston

Curious Phenomenon in Cephalonia

MR. LEDGER (p. 246) need not have had any doubt about the correctness of the information sent him by his friend about currents running from the sea into Cephalonia. It is a well-known fact, and the following account of it is from Dr. John Davy's "Ionian Islands," published in 1842, vol. i. 164:—

"The next phenomenon I have to mention is very extraordinary, and apparently contrary to the order of nature: it is the flowing of the water of the sea into the land in currents or rivulets which descend and are lost in the bowels of the earth. This occurs in Cephalonia, about a mile and a half from the town of Argostoli, near the entrance of the harbour, where the shore is composed of freestone, and is low and cavernous, from the action of the waves.

"The descending streams of salt water are four in number; they flow with such rapidity that an enterprising Englishman has erected a grist-mill on one of them with great success. I have been informed that it produces him 300*l.* a year. The flow is constant unless the mouths through which the water enters are obstructed by sea-weed. No noise is produced by the descent of the sea-water, and rarely is any air disengaged; the streams have been watched during earthquakes, and have not been found affected by them. It is stated that fresh-water is perpetually flowing through fissures in the rock from the land in the trench which has been dug for the reception of the mill-wheel, and that, when the sea-water is prevented rushing in, then the water in the trench rises higher by several inches than usual, and the water is brackish to the taste. The phenomenon has been long known to the natives. The little information I have obtained respecting these extraordinary currents I owe to my friend Dr. White, surgeon of the Second Battalion of the Rifle Brigade, collected by him when stationed in the Ionian Islands about 1840."

If Mr. Ledger's friend could give us more information it would be most desirable. I am sorry I had not an opportunity of examining the mill when I was amongst the islands in 1857.

Gateshead, January 17

R. S. NEWALL

After-Images

CAN any reader account for the following interesting phenomenon:—If I close my eyes in the presence of a strong light, so close that not a ray of light can penetrate the lids—in fact, I may generally place my hands firmly over my eyes—I can see pictures of great splendour, more beautiful than any decoration I have ever beheld, sometimes in the form of some splendid architectural design, most elaborately worked out; at others, beautiful landscapes; again, fine geometrical and other designs, as well as every conceivable form of conventional treatment, such as might be applied to carpets, or other floor decorations, iron-work, &c. I would add that all this is seen without any apparent preconceived action of the will, as sometimes, if I close my eyes with the deliberate intention of seeing any particular object, I am disappointed, though not so frequently now as when I first noticed the phenomenon a few years ago. I have sometimes seen designs positively ugly, but as a rule they are most beautiful in form and colour.

I have visited but few grand and noble buildings, and seen but little of beautiful landscapes, as I am only a humble mechanic, but I take great delight in reading descriptions of such buildings and scenes, and am a true lover of sound, substantial, and elaborate workmanship.

J. C. S.

PROFESSOR TAIT ON THE PARTITION OF ENERGY BETWEEN TWO SYSTEMS OF COLLIDING SPHERES¹

SINCE Clerk-Maxwell published, in 1860, his first grand investigation on the subject, it seems to have been taken for granted, rather than proved, that in a

¹ Abstract of Paper read to the Royal Society of Edinburgh, January 18. Communicated by permission of the Council.

mixture of great numbers of colliding spherical particles of two kinds, the ultimate state would be one in which the average energy of translation is the same for a sphere of either kind. Also that his Corollary, which extends the proposition to a mixture of many systems, is true. Further extensions have been made, the results of which have been considered as irreconcilable with the kinetic theory of gases, at least in its present form.

So far as I am aware, no really *convincing* proof of this theorem has yet been given. Maxwell's first proof is so sketchy, and involves so many inadmissible assumptions, that it cannot be looked on as more than an illustration of a truth which his deep insight had enabled him intuitively to perceive. More recent proofs depend so much on a species of analytical verbiage (under cover of which any amount of assumptions may be tacitly introduced), that, besides being totally unintelligible to any but specialists, they do not bring full conviction even to specialists themselves. What is required is plain, clear statement, and justification of every step about to be taken, such as will commend it to the careful reader, and leave no doubt on his mind as to *what* is about to be done, and *why*; though the mere details of the subsequent necessary calculation may be beyond him. Nothing does greater harm to the average reader, in the way of shaking his belief in the results of an investigation, than the use of analysis instead of, or so as to mask, thought. One may make a mistake in evaluating a definite integral, just as one may make a mistake in adding a column of figures. But when the process of forming the expression to be integrated, or of obtaining the items of the column of figures to be added, is not made fully intelligible, incredulity is very justly aroused, however we may be inclined to trust the special skill of the mere analyst or of the arithmetician in his proper sphere.

In seeking such a convincing proof, I have become from time to time suddenly aware of specially dangerous traps which (some almost obvious, others extremely difficult to detect) abound in this particular region of inquiry. Some of these will appear in what follows. Hence I determined not to be content with anything short of absolutely pointing out the nature of, and the reason for, every step; so that even those who cannot follow the step itself may fully understand *why* it has been taken, and be in a position to judge of its legitimacy.

Limits of space forbid my giving all this in an abstract, so that I must confine myself to a very condensed statement.

For reasons given, we assume the truth of the "error-law" distribution of speeds in any one system of spheres. This will be called the "special" state.

When two systems are mixed, we assume the mixture to be complete; and, on account of the small fraction of the whole number of particles (one from each, or one from either, system), which are at any time in collision, *and of the perfect freedom of collision between any two assigned particles* (this is a point of special importance), we assume that each system, by its internal collisions, maintains its own "special" state. Hence in our investigations the collisions of two particles of the same system need not be attended to. Their sole function has been assigned, and we assume that they accomplish it.

But it is most distinctly to be understood that the above assumptions are absolutely necessary to the prosecution of the inquiry in the manner adopted; and, therefore, to whatever result it may legitimately lead, that result is not to be held as accurate if any of them be departed from. Thus the extensions of Maxwell's Theorem, given by Boltzmann and others, must not be considered as legitimate extensions of that Theorem and its corollaries unless, in the collisions between complex particles, the mechanism of each degree of freedom of any one such particle has perfectly free access for collision with that of the corresponding as well as with that of the non-cor-